Family Formation and the Great Recession

by

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Abstract

This paper studies how exposure to recessions as a young adult impacts long-term family formation in the context of the Great Recession. Using confidential linked survey data from U.S. Census, I document that exposure to a 1 pp larger unemployment shock in the Great Recession in one's early 20s is associated with a 0.8 pp decline in likelihood of marriage by their early 30s. These effects are not explained by substitution toward cohabitation with unmarried partners, are concentrated among whites, and are notably absent for individuals from high-income families. The estimated effects on fertility are also negative but imprecisely estimated. A back-of-the envelope exercise suggests that these reductions in family formation may have increased the long-run impact of the Recession on consumption relative to its impact on individual earnings by a considerable extent.

Keyword*:* Marriage, Fertility, Great Recession, Equivalence Scales

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1 Introduction

How does exposure to recessions as a young adult impact family formation in the long run? Several entries of the economics literature have documented that early exposure to recessions can have long-term negative ("scarring") effects on human capital (Stuart, 2020) and earnings over the life cycle (Kahn, 2010; Oreopoulos et al., 2012). The scarring effects of recessions on non-economic outcomes such as marriage and fertility have been studied comparatively less, particularly for more recent birth cohorts who were exposed to the Great Recession as young adults[1]. *A priori*, these effects are ambiguous: worsened economic conditions may decrease the opportunity cost of forming a family for exposed individuals but may also make them less attractive as potential partners. Moreover, the potential implications of these effects on family formation for the long-run impact of recession exposure on consumption are not well understood. These implications may be important to consider, as the literature that has studied household equivalence scales suggests that household economies of scale in consumption are large (Nelson, 1988; Browning et al., 2013). Thus, considering only *individual* income scarring effects may overstate or understate the long-run effect of recessions on consumption and economic well-being if they also considerably impact family formation.

This paper studies the research question in the context of the financial crisis of 2007-08 and the subsequent Great Recession. The Great Recession induced severe shocks to employment across the country, with the intensity of these shocks varying considerably across labor markets. This spatial heterogeneity has allowed researchers to use U.S. local labor markets as a laboratory in which to study the effects of the Recession (Yagan, 2019). I follow this methodology and conduct my analysis using a novel dataset comprised of linked confidential data from U.S. Census. New linkage rules and methods allow for individuals in the yearly American Community Surveys (ACS) to be linked to themselves in the 2000 Census, thus providing a substantially more recent and granular proxy of one's likely location at the time of the Great Recession than is available in public-use data — in addition, this linkage allows one to view socioeconomic background information for some young adults in the ACS who have left their childhood home, which cannot be done in public-use data. I focus my analysis on the birth cohorts of 1983 through 1987. These individuals would have been between ages 21 and 25 in 2008, thus preempting the large majority of their family formation while also enabling me to observe their outcomes into their early-to-mid 30s.

[1] Analyses of older cohorts suggest that worse labor markets as a young adult depresses fertility (Currie and Schwandt, 2014) and may either delay or encourage marriage among women (Gutiérrez-Domènech, 2008; Kondo, 2012; Maclean et al., 2016; Schwandt and VonWachter, 2020).

I measure recession severity at the commuting zone (CZ)[2] level and use individuals' 2000 CZ of residence as a proxy for their location at the time of the recession. Leveraging the aforementioned variation in shock severity in a generalized difference-in-differences specification, I find that exposure to a 1 pp larger unemployment shock in the Great Recession is associated with a 0.8 pp reduction in marriage rates in an individual's early 30s. These effects cannot be explained by substitution toward cohabitation with unmarried partners and appear to be driven by reductions in partnering in the first place as opposed to increased marital dissolution. Heterogeneity analyses suggest that these effects are concentrated among whites but are notably absent for individuals from high-income families, suggesting that the Great Recession may have played a part in exacerbating socioeconomic disparities in family structure. The estimated effects on fertility are also negative but imprecisely estimated. These findings suggest that the focus of the economic literature on the scarring effects of recessions on individual earnings may understate the full impact of recessions on individual consumption. Using the estimates of the impacts of the Great Recession on marriage in conjunction with individual earnings scarring effects and a range of estimates of household equivalence scales reported in the economics literature (Fernández-Villaverde and Krueger, 2007) in a back-of-the-envelope exercise, I find that accounting for reductions in household formation may increase the scarring effect of recessions on personal consumption by 30 to 50 percent.

Related literature

This paper contributes to several strands of the economics literature, most directly to the literature that studies the lingering effects of recession exposure on individual outcomes. Graduating into recessions is associated with substantially depressed earnings for at least 10 years (Kahn, 2010; Oreopoulos et al., 2012; Altonji et al., 2016). While some work has suggested that these scarring effects fade after approximately a decade, other work has found that the effects can reemerge later in life and be near-permanent (Schwandt and von Wachter, 2019; Stuart, 2020). As data availability increases, a growing literature has studied the effects of the Great Recession by leveraging spatial variation in the shocks induced by it, generally finding that the scarring effects associated with these shocks are severe (Yagan, 2019; Rinz, 2019; Rothstein, 2020).

This literature has focused primarily on earnings and wages as the outcome variables of interest, and studies on the scarring effects of recessions on non-economic outcomes are

[2]Commuting Zones are a frequently-used notion of local labor markets and groups of counties characterized by strong economic ties. There are 722 CZs in the United States.

markedly less common. Perhaps most complementary to this paper is contemporaneous work by Schwandt and VonWachter (2020), who find using public-use American Community Survey data that cohorts who entered the labor market in the recession of the early 1980s experienced higher mortality and lower rates of earnings, marriage and fertility throughout their 30s and 40s. In addition to focusing on different recessions, our results differ qualitatively: the authors find that graduation into a recession is initially associated with *higher* rates of marriage, but the direction of the relationship gradually changes until becoming negative by age 40. In contrast, the effects I find are consistently negative and emerge in the late 20s, suggesting that changes in the marriage market between the 1980s and now may have affected the incidence of recessions on marriage in important and interesting ways.

I also offer a methodological contribution in dealing with the problem of individuals in the ACS potentially having moved from their state of birth, which is typically used to infer one's likely location at the time of a recession. While Schwandt and von Wachter (2019) and Schwandt and VonWachter (2020) use a double-weighted unemployment rate technique to address the issue, I link individuals in the ACS to themselves in the 2000 Census to infer their likely location at the time of the Great Recession. In addition to providing a higher-quality location proxy than their state of birth, comparing the locations of individuals in the 2007 ACS to where they were in 2000 allows for a precise quantification of how severe the migration problem is likely to be in the context of the Great Recession, and I find that any bias induced by migration is likely to be minimal. Moreover, linkages to the 2000 Long-Form Census allow me to observe detailed parental covariates for adults in the ACS who have left their childhood home and test for heterogeneity in effects by socioeconomic background, which with public-use data is impossible.

In focusing on family formation, this paper speaks to the large literature that has studied the relationship between economic conditions and family structure. Several papers have investigated the relationship between economic shocks and marital dissolution: Doiron and Mendolia (2011) find in British panel data that job losses increase the likelihood of divorce, and Hellerstein and Morrill (2011) find similar results using state-level divorce and unemployment rates in the U.S. An especially large literature has focused on the relationship between the business cycle and fertility: Schaller et al. (2020), for instance, finds an overall positive relationship between income growth and fertility across U.S. states from the years 1937-2016, and other work (Schaller, 2016) suggests that improved male labor market conditions are particularly pro-natal[3]. The literature on how labor market conditions affects marriage for

[3]For a review of the literature on recessions and birth rates in developed countries, see Sobotka et al.

men, women, or both in particular is relatively small, with Blau et al. (2000) being one of the seminal contributions. Kearney and Wilson (2018) and Autor et al. (2019) study labor market shocks on the marriage patterns of young men and find mixed results. Neither study focuses on particularly long-term effects — in contrast, Schaller (2013) finds using state-level unemployment rates and vital statistics that increased unemployment rates depress marriage rates for three years, with no increase in marriage rates afterward to compensate.

Most closely related to my paper in this literature are Currie and Schwandt (2014) and Kondo (2012), who respectively find that worsened labor market conditions for women in their early 20s depresses fertility and either permanently depresses marriage (the former paper) or temporarily lowers marriage ages without affecting completed marriage later in life (the latter). I contribute to the literature on economic conditions and family formation by focusing on a more recent set of birth cohorts and by linking it to the literature on recession earnings scarring effects. In particular, I do this through demonstrating that reductions in marriage may substantively magnify the adverse effects that recessions have on economic well-being that go beyond their effects on personal income[4]. In doing so, this paper contributes to the literature that has studied household equivalence scales in consumption (Nelson, 1988; Lewbel, 1989; Browning et al., 2013) by using estimates of these scales in a new context.

The remainder of the paper is organized as follows: Section 2 introduces my data and empirical strategy. Section 3 presents my empirical results, and Section 4 discusses the potential implications of these results in regard to the scarring effects of recessions on income and consumption. Section 5 discusses potential avenues for future research before concluding.

2 Data and Methodology

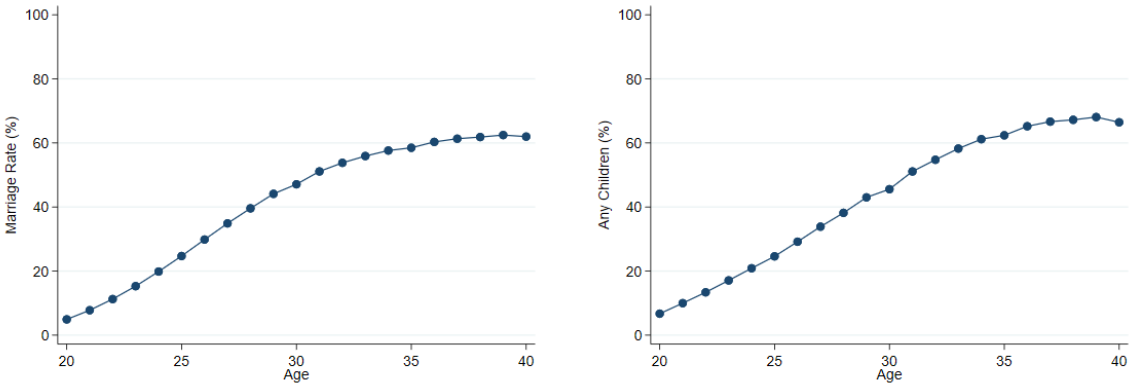
2.1 Sample Description

To estimate the effects of the Great Recession on the long-term family formation of young adults, I use newly available linked confidential data consisting of the 2000 Decennial Census

(2011). For a literature review on economic causes of changes in marriage and divorce, refer to Stevenson and Wolfers (2007). See Schneider (2017) for a sociological review of related issues.

[4]Engdahl et al. (2020) make a related point in the context of the early 1990s recession in Sweden, where they show that low-GPA women who graduated into the recession had higher welfare receipts later in life despite no lasting earnings effect due to increased rates of single motherhood. Relative to their study, I do find an earnings scarring effect and relate consumption implications from decreased family formation directly to it.

Figure 1: Family Formation over Age



(a) Marriage Rates (%)

(b) Any Children in Household (%)

Notes: Data from 2005-2018 American Community Survey.

and the 2005-2018[5] waves of the American Community Surveys. In addition to containing considerably richer geographic information for the respondents, individuals in both these datasets have been linked to the Social Security Administration NUMIDENT file using the U.S. Census Bureau's Person Identification Validation System, which allows me to assign Protected Identification Keys (PIKs; essentially scrambled Social Security Numbers) to the large majority of individuals in the surveys[6].

While other papers have made use of these data as repeated cross-sections, recent regulation changes regarding linking mandatory Census surveys allow for respondents in these surveys to be linked to themselves over time[7]. With PIKs being stable over time and the large majority of U.S. citizens appearing in either the short or long-form 2000 Census, this allows me to link individuals in the ACS to themselves in the 2000 Census, effectively forming a two-year panel for a 1% sample of the U.S. over the years 2005 to 2018. For individuals

[5]The 2001-2004 waves of the survey only sampled a limited number of counties and as a result suffer from representativeness issues. As such, they are not fully comparable to the 2005-onward waves and are not used in this analysis.

[6]The PVS is Census Bureau's system for searching and verifying Social Security Numbers or Protected Identification Keys for person records in demographic surveys and/or censuses. This system first attempts to match individuals in surveys to SSA records by using address information — if this fails, individuals are matched via name and date of birth. A more comprehensive review of this procedure and its performance can be found in Mulrow et al. (2011)

[7]Conversations with Census Research Data Center administrators suggest that this change happened some time in the 2019 calendar year.

Table 1: ACS Linkage Rates

Dataset	PIK Assignment Rate	Census Link Rate
2005 ACS	90.1	61.0
2006 ACS	88.4	60.7
2007 ACS	87.7	59.7
2008 ACS	87.7	57.9
2009 ACS	86.4	56.8
2010 ACS	92.2	57.4
2011 ACS	88.0	55.1
2012 ACS	89.1	55.8
2013 ACS	89.7	56.0
2014 ACS	90.1	55.8
2015 ACS	90.4	55.5
2016 ACS	90.0	55.4
2017 ACS	90.0	55.0
2018 ACS	91.0	55.5

Notes: Data from 2000 Census and 2005-2018 American Community Surveys. Table reports linkage rates of individuals in 2005-2018 ACS to their protected identity keys (PIKs) and to themselves in the 2000 Census. Statistics are for 1983-1987 birth cohorts. Linkage rates rounded for disclosure avoidance purposes.

whose households were surveyed in the long-form Census, this linkage allows me to observe detailed family background information among adults in the ACS who no longer live with their parents, overcoming a key limitation of the public-use version of the data. For the entire linked sample, I can observe detailed information about where individuals lived in 2000, which affords a higher-quality proxy for one's location at the time of the Great Recession than their state of birth.

My baseline sample consists of individuals born 1983-1987 who are observed at some point in the ACS. These birth years translate to being aged between 21 and 25[8] in 2008, thus ensuring that they were exposed to the Great Recession before the bulk of their family formation activity. At the same time, individuals in the sample can be observed up to age 35 in the 2018 ACS, at which point a large share of family formation is likely to have taken place: Figure 1 displays life-cycle profiles for family formation in the U.S. over 2005-2018 and demonstrates that the large majority of marriage formation and fertility takes place after the early 20s before leveling off in the late 30s. Individuals living in Group Quarters in the 2000 Census are dropped as are any groups of individuals in any survey year who are

[8] Currie and Schwandt (2014) use a very similar age range (20-24) as the critical timing in their analysis.

linked to the same PIK.

After performing the linkages and applying the sample restrictions listed above, I am left with over two million observations of the 1983-1987 birth cohorts over the 2005-2018 period. Table 1 reports the rates at which I can both assign individuals in the ACS PIKs and link them to themselves in the 2000 Census. I am able to obtain year-2000 location and at least basic family background information for over half the sample in any given year. Table 2 reports how basic demographic and socioeconomic characteristics of my ACS sample change after removing unlinked individuals in either step. Monetary variables are deflated to 2012 dollars using the PCE deflator. The table suggests that the linkage process does induce some selection, with the economic indicators of the sample improving somewhat after the linkage procedure. In particular, whites and individuals from the Midwest become overrepresented after sample restrictions. This is not surprising, as the PIK system relies most heavily on individual addresses to make linkages; thus, individuals who do not have stable addresses are more likely to be unmatched and subsequently dropped. To address this selection issue, I calculate the ex-ante probability of being included in the baseline sample by state, gender, and race (white, black, Hispanic or Asian) and modify individual sample weights by dividing them by these probabilities[9].

2.2 Empirical Strategy

I estimate the effects of exposure to the Great Recession using a generalized difference-in-differences specification that compares family structure for individuals who were in labor markets that were more or less severely shocked by the Great Recession (first difference) and individuals who are at different points in the life cycle or are measured at different points in time (second difference). The specification takes individuals who were in their early 20s in 2008 and leverages the spatial heterogeneity in recession intensity to estimate how exposure to worse shocks influences family formation up to the mid 30s, by which point the bulk of family formation is likely to have been completed. In particular, I use the following regression specification:

$$y_{iact} = \beta_t S_c + \boldsymbol{\alpha} \boldsymbol{X}_i + \theta_a + \gamma_c + \delta_t + \varepsilon_{iact}, \tag{1}$$

[9]Using the original sample weights instead does not change the sign or significance of the results.

Table 2: Sample Summary Statistics

Variable	ACS	ACS (PIK Linked)	ACS (Census Linked)
Age	26.14	26.18	25.97
	(4.24)	(4.24)	(4.25)
Male	50.88	50.18	50.62
	(49.99)	(50.00)	(50.00)
White	70.29	71.78	80.79
	(45.70)	(45.01)	(39.40)
Black	14.04	13.89	8.74
	(34.74)	(34.59)	(28.24)
Asian	5.88	5.74	3.86
	(23.52)	(23.27)	(19.27)
Hispanic	19.98	16.94	13.02
	(39.99)	(37.51)	(33.66)
Married	29.38	29.90	29.41
	(45.55)	(45.78)	(45.56)
Income ($1,000)	24.03	24.93	26.30
	(27.09)	(27.63)	(28.38)
Bachelor's Degree	0.26	0.27	0.31
	(0.44)	(0.44)	(0.46)
Associate's Degree	0.33	0.35	0.39
	(0.47)	(0.48)	(0.49)
High School Degree	0.88	0.90	0.93
	(0.33)	(0.30)	(0.25)
In Northeast	17.39	17.40	17.13
	(37.91)	(37.91)	(37.67)
In Midwest	20.90	21.87	23.87
	(40.66)	(41.34)	(42.63)
In Southeast	36.94	36.79	35.71
	(48.26)	(48.22)	(47.91)
In West	24.77	23.94	23.29
	(43.17)	(42.67)	(42.27)
N	3963000	3542000	2252000

Notes: Data from 2000 Census and 2005-2018 American Community Surveys. Statistics are for 1983-1987 birth cohorts. Standard deviations in parenthesis. Sample sizes rounded for disclosure avoidance purposes. Column 2 reports variable means and standard deviations for entire ACS sample. Column 3 reports variable means and standard deviations for ACS sample linked to PIKs. Column 4 reports variable means and standard deviations for ACS sample linked to 2000 Decennial Census.

where y_{iact} is some outcome variable of interest, such as marital status, for individual i, who was in commuting zone c in 2000, and surveyed at age a in year t. The vector $\boldsymbol{X}_i$ contains categorical race (Black, Asian, Hispanic), education (a dummy for completing a four-year degree[10]), and sex variables, and ε_{iact} is an error term. Age and CZ fixed effects θ_a, γ_c strip out life-cycle and geographic trends in the variable of interest, and time fixed effects δ_t account for year-specific factors that may influence individual behavior at the population level. To account for potential geographic differences in life-cycle patterns in the outcome variable of interest, the time fixed effects δ_t in the regression are interacted with Census division dummies[11].

Following Yagan (2019), S_c is the 2007-2009 change in unemployment rate observed in individual i's CZ of residence in 2007[12]. These shocks are visualized in Figure 2 — areas in the Rust Belt, the West, and the Southeast experienced particularly strong shocks, while areas such as the Great Plains and the Northeast were spared in comparison. The β_t terms thus are the coefficients of interest and have the interpretation of the effect on some outcome at time t of living in 2007 in a labor market that experienced a one percentage point larger unemployment shock in the Great Recession. One limitation of my data is that the 2007 CZ of residence for survey respondents is not directly observable, so I use their 2000 CZ of residence as a proxy[13]. Looking at the 2007 ACS allows a direct evaluation of how good this approximation is: I find that approximately 72% percent of my sample in 2007 lived in the same CZ as in 2000. Individuals who moved did not appear to move to substantially different locations in terms of recession exposure, with the average difference in shock intensity being -0.1 points, less than 1/15th of a standard deviation in shocks received by the sample[14].

[10]The cohort selection is such that individuals in my sample should have completed the bulk of their post-secondary schooling by the time the Great Recession occurred. Nonetheless, it is possible that the Great Recession depressed human capital accumulation in my sample through decreasing financial resources with which to fund college attendance. If this was one mechanism that decreased marriageability, then including college attainment as a control variable should bias β_t upward.

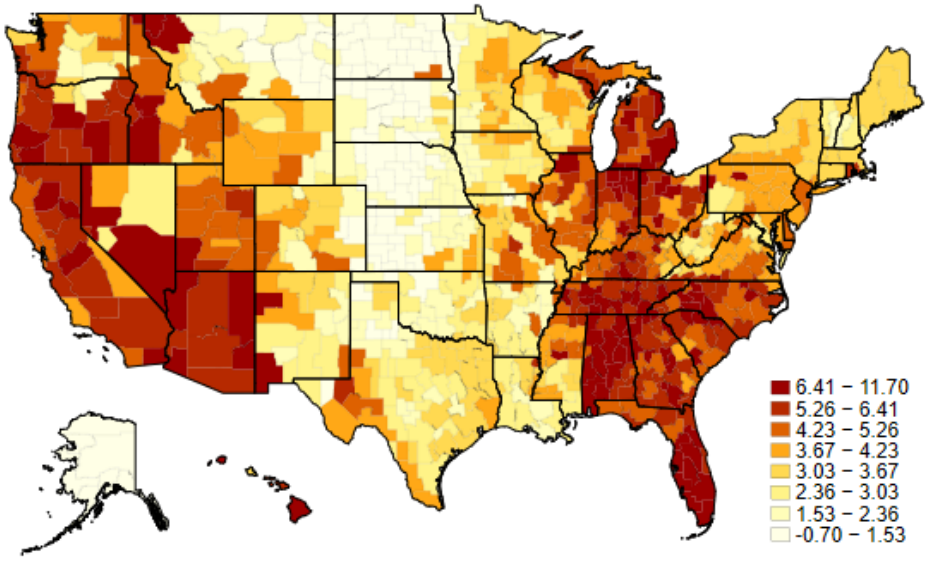
[11]There are nine Census divisions; refer to Appendix A for exact groupings of states. I prefer division-by-year fixed effects to state-by-year fixed effects because within-state variation of CZ shock intensity is quite limited: state fixed effects can explain approximately 70% of the variation in CZ unemployment shocks. The corresponding statistic for division fixed effects is 40%.

[12]CZ shocks are computed by taking county-level unemployment rates from the yearly BLS Local Area Unemployment Statistics series and then aggregating to CZs with population weights before averaging across months to form a yearly CZ measure.

[13]In addition to CZ being a more granular level of geography than is available in public-use data, the year-2000 location is likely to be non-negligibly more accurate as a proxy than is birth location. While birth CZ is not directly available in the data, over 20% of individuals in the 1983-1987 cohorts had moved from their birth state by 2000.

[14]A considerable part of this likely comes from the fact that 45% of movers are still observed within their

Figure 2: Great Recession CZ Shocks



Notes: Data from BLS LAUS series.

This, in conjunction with the large majority of individuals in 2007 being observed in their year-2000 CZ, suggests that any noise injected into the results by migration flows is likely to be small.

To facilitate the inclusion of CZ fixed effects, I normalize $\beta_{2007} = 0$. Thus, the estimates of β_t are the effects of a 1 pp larger Great Recession shock on individuals at time t minus its effect in 2007. Given the timing of the Great Recession, a reasonable hypothesis is that effects in year 2007 should be close to zero — the same logic offers a falsification test that β_{2006} and β_{2005} should be zero as well. The identifying assumption is the familiar parallel trends assumption, or that the intensity of the Great Recession unemployment shock received by a CZ was not systematically related to its pre-recession trend in the outcome variable of interest. Observing estimates of β_{2006} and β_{2005} allow me to evaluate the validity of this assumption — in addition, as other papers studying the Great Recession have used the same CZ shocks I use here, I conduct an additional validation test where I verify that my specification can reproduce effects of Great Recession shocks on earnings reported in other entries in the literature. I cluster standard errors by year-2000 CZ to allow for arbitrary serial correlation of error terms within CZs.

year-2000 state. Unsurprisingly, college attendance appears to be an important driver of these moves: 71.3% of movers had attended at least some college, compared to only 58.3% of non-movers.

Figure 3: Effects of Great Recession Exposure on Earnings and Marriage by Year

(a) Wage Earnings

(b) Married

Notes: Figures present estimates of β_t from equation (1). Standard errors clustered at the year-2000 CZ level, and 95% confidence intervals shown. The sample includes individuals of the 1983-1987 birth cohorts observed in the 2005-2018 ACS and linked to themselves in the 2000 Decennial Census; see text for details. All specifications include demographic controls, CZ fixed effects, age fixed effects, and year fixed effects interacted with Census division.

3 Results

Estimates of β_t for wage earnings and marital status are presented in Figure 3. For earnings (Figure 3a), exposure to a one percentage point larger CZ unemployment shock between 2007 and 2009 is associated with an approximately $700 reduction in earnings in the long run. In addition to their being no visible pretrends, other entries in the literature provide support for the face validity of this effect. Yagan (2019) finds that exposure to a 1 pp larger shock for individuals who were age 25-54 is associated with a long-term reduction in earnings of $1,000; Rinz (2019) replicates this result and conducts subgroup analyses of the effects of the Recession on earnings over different generations (Millenials, Gen-X, Baby Boomers, and the Silent Generation). He estimates a long-run effect of exposure to a 1 pp larger shock of approximately $600 for Millenials, suggesting that the earnings effects I find are reasonable[15].

Figure 3b reports the estimates of β_t for the outcome variable of whether the respondent is currently married. As before, pre-trends are insignificant and flat. The estimates are noisier in general than those for wage earnings, but for 2013-onward the estimates suggest

[15]An additional result in the paper is that recession shock impacts on employment for Millenials are transitory, which I also find in Figure B.1.

with at least a marginal level of significance that exposure to a 1 pp larger unemployment shock in the Recession is associated with, on average, an approximately 0.5 pp decrease in the probability the affected individual is married.

While flat, the pre-trends in Figure 3b are not especially informative because only a very small proportion of the sample would have been married in 2005 or 2006. As an additional test, I run the same specification on the birth cohorts of 1973-1977, who would have been much more at risk of being married in the pre-period. The results of this exercise may be found in Figure B.2: while the earnings scarring effect is strongly significant and similar to the effect found in Yagan (2019), the effects on marriage are only slightly negative and are statistically indistinguishable from zero. Interpreting the marriage effect for these cohorts is made more complicated by the fact that a considerable number of unions may have been formed in the pre-period, so the effect of the recession on marriage depends on both its impact on divorce[16] and on new marriage formation. However, the results at least provide evidence that marriage rates were not systematically trending down in CZs that were hit harder by the recession beforehand.

A potential source of noise in the estimates is that the effects of the Great Recession on marriage likely depend more on an individual's point in the life cycle (i.e. their age) than the calendar year. To consider this possibility, I run an alternate version of (1) that interacts Great Recession shocks with age categories instead of calendar year:

$$y_{iact} = \beta_a S_c + \boldsymbol{\alpha} \boldsymbol{X}_i + \theta_a + \gamma_c + \delta_t + \varepsilon_{iact}, \tag{2}$$

so β_a has the interpretation of the effect on some outcome at age a of living in 2007 in a labor market that experienced a one percentage point larger unemployment shock in the Great Recession. Age fixed effects θ_a are now interacted with Census division dummies instead of the year fixed effects so that the specification continues to compare individuals within reasonably comparable geographic areas. To further increase the precision of the estimates, I group age into six three-year bins (18-20, 21-23, 24-26, 27-29, 30-32, 33-35) and normalize $\beta_{18-20} = 0$.

Table 3 presents the results of this exercise in the second column. Reassuringly, the estimates do not detect any significant or substantive impact of the Recession on marriage rates at age 21-23. I do not detect any significant effects until age 30, at which point the estimates become larger in magnitude than those in Figure 3b and considerably more

[16] Evidence suggests that the Great Recession may have actually *decreased* divorces (Cohen, 2014).

Table 3: Effects of Great Recession Exposure on Marriage

VARIABLES	(1) Married	(2) Married	(3) Cohabiting	(4) Married
Age 21-23	0.017	0.017	-0.256	-0.145
	(0.185)	(0.171)	(0.242)	(0.235)
Age 24-26	-0.293	-0.293	-0.682	-0.405
	(0.346)	(0.334)	(0.344)	(0.373)
Age 27-29	-0.453	-0.453	-0.703	-0.374
	(0.309)	(0.342)	(0.269)	(0.372)
Age 30-32	-0.644	-0.644	-0.692	-0.446
	(0.248)	(0.276)	(0.217)	(0.336)
Age 33-35	-0.800	-0.800	-0.651	-1.291
	(0.224)	(0.243)	(0.210)	(0.343)
Observations	2252000	2252000	2252000	537000
R^2	0.16	0.16	0.178	0.157
Cluster	CZ	State	CZ	CZ
Additional Controls	NO	NO	NO	YES

Notes: Table presents estimates of β_a from equation (2). Standard errors in parentheses. The sample includes individuals of the 1983-1987 birth cohorts observed in the 2005-2018 ACS and linked to themselves in the 2000 Decennial Census; see text for details. All specifications include demographic controls, CZ fixed effects, year fixed effects, and age fixed effects interacted with Census division. Observation counts rounded for disclosure avoidance purposes.

precisely estimated. The baseline specification suggests that a 1 pp larger unemployment shock in the Recession reduces marital probability by age 33-35 by 0.8 percentage points. To put this marginal effect in perspective, the average marriage rate among individuals in the analysis sample aged 33-35 is 54.6%. The 25th and 75th percentiles of CZ shocks received in the Great Recession were 3.54 and 5.88 points, respectively — multiplying the difference of 2.34 by the marginal effect and dividing by the base rate suggests that an individual being exposed to a 75th-percentile shock as opposed to a 25th-percentile shock would reduce their marriage probability by about 3.4%. A similar exercise with 10th and 90th percentile shocks

(2.94 and 6.54 points) yields a reduction of 5.3%.

Comparing the effects found here to those found by Schaller (2013) is interesting, as she finds stronger effects in marriage rates at the state level in the years shortly after a recession and null long-term effects. A potential driver of the difference between our findings is my focus on individuals who were age 21-25 when the Recession happened, as this group would have been unlikely to marry in their early-to-mid 20s regardless of the Recession. The scarring effect of the Recession instead shows up in their early 30s, at which point more marriages are being formed. My estimates are comparable, however, to other papers that have studied the scarring effects of worse labor markets in early adulthood on non-economic outcomes for older birth cohorts in the public-use ACS: Currie and Schwandt (2014) and Schwandt and VonWachter (2020) respectively estimate scarring effects of a 1 pp higher unemployment rate at the state level on long-run marriage rates of -0.65 and -0.5 points, both of which fall in the confidence interval of my estimate.

The remaining columns of Table 3 conduct robustness tests. Column 2 accounts for potential spatial correlation of error terms among individuals from different CZs within the same U.S. state by clustering standard errors at the year-2000 state level. The size of the standard errors is not substantively changed after making this adjustment. Column 3 expands the outcome variable to include individuals who are living with an unmarried partner. A natural concern with the results for marital status is that marriages are frequently expensive, so couples affected by the Recession may simply substitute away from formal marriage to cohabitation while still forming households at the same rate as before. However, I find a statistically and substantively significant effect of the Great Recession on cohabitation as well, and estimates of β_{30-32} and β_{33-35} are not statistically distinguishable from those of the baseline specification. Interestingly, the effects are equally as strong for individuals in their mid-to-late 20s, which suggests that the Great Recession's negative effect on marriage among individuals in their 30s may have partly come from discouraging their forming partnerships earlier on.

The final column of Table 3 includes an additional vector of control variables available for individuals who were linked to themselves in the Long-Form Census. Since the cohorts I use would have been at most 17 in 2000 and thus still attached to their parents, I can observe detailed socioeconomic background information for individuals linked to Long-Form. I include controls for parental marital status, education, income, homeownership, house value, and a dummy for whether the household head worked in construction or manufacturing[17]

[17]This serves as a proxy for whether the respondent was likely to have experience a parental layoff in the

in 2000 in $\boldsymbol{X}_i$. This specification returns qualitatively similar results to the baseline results, and while the estimates of β_{30-32} and β_{33-35} differ slightly in their magnitudes, neither can be rejected from being equal to the initial estimates with a strong degree of confidence.

While the empirical specification attempts to compare individuals from similar CZs in terms of geography, one may be concerned that even within geographic groups the CZs that received larger shocks in the Great Recession differed from those that did not in ways that may contaminate the results. To assess this issue, I run additional specifications where I interact additional pre-recession CZ characteristics with age groups and include these as supplementary control variables. I consider pre-recession rates of marriage, college attainment, employment, and poverty to see how accounting for CZ-level differences in demographics, human capital, and economic conditions might influence the results. The results of this exercise are presented in Table B.1: in all cases, the results are qualitatively unchanged, and estimates of β_{33-35} fall within the confidence interval of the baseline estimate.

Finally, Table B.2 reports the results of additional analyses. Columns 1 and 2 alter the education controls used in the regression by either including dummies for all different education levels or none at all; in both cases, the results change little. Column 3 focuses again on the individuals linked to themselves in the 2000 Long-Form but does not include the additional controls available in it, which demonstrates that the differences in estimates between columns 1 and 4 of Table 3 come more from the differences between the samples than they do the additional controls. Columns 4 and 5 present recession impacts on wage earnings and employment as estimated by (2). Finally, column 6 assesses the importance of migration in terms of biasing the results by using the entire ACS sample (regardless of whether individuals are linked to themselves in the 2000 Census) and by using one's *current* CZ as a proxy for their year-2007 CZ instead of their year-2000 CZ. While the point estimates cannot be distinguished as substantially different from the preferred estimates, they are larger in magnitude and would suggest significant marriage impacts for individuals in their mid-to-late 20s, in contrast to the baseline results.

3.1 Heterogeneity

I next test for heterogeneity in the effects of the Great Recession on marriage. I do this by conducting subgroup analyses based on race/sex categories for the whole sample and by

Recession, as construction and manufacturing were the two most-affected industries by a wide margin.

Table 4: Demographic Heterogeneity in Effects

VARIABLES	(1) Married	(2) Married	(3) Married	(4) Married
Age 21-23	0.125	-0.135	-0.051	0.486
	(0.221)	(0.228)	(0.213)	(0.296)
Age 24-26	-0.243	-0.431	0.147	0.434
	(0.364)	(0.350)	(0.308)	(0.305)
Age 27-29	-0.228	-0.447	0.139	0.339
	(0.322)	(0.227)	(0.321)	(0.357)
Age 30-32	-0.451	-0.471	0.104	0.061
	(0.243)	(0.174)	(0.346)	(0.356)
Age 33-35	-0.686	-0.762	0.257	-0.184
	(0.258)	(0.236)	(0.486)	(0.468)
Sample	W Men	W Women	NW Men	NW Women
Observations	873000	863000	258000	258000
R^2	0.175	0.177	0.115	0.120

Notes: W: non-Hispanic white. NW: nonwhite or Hispanic. Table presents estimates of β_a from equation (2). Standard errors clustered at the year-2000 CZ level and are in parentheses. The sample includes individuals of the 1983-1987 birth cohorts observed in the 2005-2018 ACS and linked to themselves in the 2000 Decennial Census; see text for details. All specifications include demographic controls, CZ fixed effects, year fixed effects, and age fixed effects interacted with Census division. Observation counts rounded for disclosure avoidance purposes.

parent income quartile[18] for the sample linked to the 2000 Long-Form.

Table 4 presents estimates of β_a broken down by race (non-Hispanic white and other) and sex. The immediate result is that effects appear to be concentrated among white men and women — all estimates of β_a for nonwhites are noisily estimated. Notably, the effects for women and men in both racial groups are approximately equal, which likely stems from comparable labor force participation rates among men and women in these cohorts and my

[18]Quartiles are computed within-cohort using the entire 2000 Census. Note that since quartiles are computed before the ACS linkage is made, the number of individuals in each quartile in the analysis sample need not be the same.

empirical specification's use of a shock that did not vary by gender and had similar impacts on the earnings of men and women (Yagan, 2019; Rinz, 2019)[19]. Notably, the estimated effects on wage earnings are negative for all demographic groups (Table B.3, though note that the earnings effects for nonwhites are quite noisy), suggesting potential heterogeneity in how earnings factor in family formation decisions across different races.

Table 5 conducts the exercise over parent income quartiles, with the main finding being that the negative effects of the Great Recession on marriage at age 33-35 are *not* present for individuals whose parents were in the top income quartile in 2000[20]. There is slight evidence of a decline in marriage rates at 30-32 for individuals with top-quartile parents, which may point to the Great Recession causing these individuals to delay marriage temporarily relative to their less wealthy counterparts. Taken together, the results suggest that the Great Recession had the strongest negative effect on marriage for whites from poor to middle-class backgrounds and exacerbated socioeconomic gaps in marriage formation.

The heterogeneity over parental socioeconomic status also suggests potential mechanisms driving the results. Young adults may prefer to reach (or partner with those who have) a certain level of financial success and stability before forming relationships and/or marital unions. Exposure to harsher labor market conditions during the recession likely impeded the ability of young adults to achieve this, but altruistic parents may have been able to offer assistance. Of course, these same parents were likely to experience resource shocks themselves during the downturn. That young adults with top-quartile parents did not seem to change their marital behavior in the face of worse recessionary shocks is consistent with a world where a certain level of intergenerational insurance was exercised by those able to give it, and beneficiaries of these transfers were more able to mitigate the recession's effects on their economic well-being. In support of this is that the scarring effect of the recession on wage earnings appears to fade for children with above-median-income parents (Table B.4)[21].

[19] In light of the Beckerian marriage model and other literature that has looked at the impacts of gender-specific shocks and found asymmetric responses over sex in marriage behavior in response to them (Kondo, 2012; Schaller, 2013), these results may seem surprising. That the shocks I consider here are not gender-specific is important: given that I am looking at the same treatment over both sexes and that individuals in my sample would be most likely to marry one another, a man in the sample not marrying as a result of the shock ought to result in a comparable effect for a woman in the sample. Thus, a degree of symmetry in the coefficients is actually to be expected.

[20] In general, though, the coefficients for earlier ages should perhaps be taken with some caution given the size of the standard errors. Notably, some of the estimates for individuals with bottom-quartile parents are positive in sign though cannot be rejected from being negative as well. This may be indicative of poorer individuals initially entering lower-quality marriages following an economic downturn that then dissolve.

[21] I leave to future work a more thorough study of how the effects of the Great Recession varied over the parent income gradient.

Table 5: Socioeconomic Heterogeneity in Effects

VARIABLES	(1) Married	(2) Married	(3) Married	(4) Married
Age 21-23	-0.556	-0.206	0.052	0.061
	(0.475)	(0.384)	(0.292)	(0.290)
Age 24-26	-0.472	-0.568	-0.552	-0.012
	(0.578)	(0.479)	(0.381)	(0.510)
Age 27-29	0.574	-0.882	-0.717	-0.432
	(0.646)	(0.456)	(0.438)	(0.531)
Age 30-32	0.356	-0.527	-0.717	-0.715
	(0.543)	(0.476)	(0.495)	(0.428)
Age 33-35	-1.688	-1.852	-1.228	0.081
	(0.685)	(0.657)	(0.599)	(0.702)
Parent Income Quartile	1	2	3	4
Observations	110000	142000	152000	132000
R^2	0.123	0.145	0.177	0.214

Notes: Table presents estimates of β_a from equation (2). Standard errors clustered at the year-2000 CZ level and are in parentheses. The sample includes individuals of the 1983-1987 birth cohorts observed in the 2005-2018 ACS and linked to themselves in the 2000 Long-Form Decennial Census; see text for details. All specifications include demographic controls, year fixed effects, CZ fixed effects, and age fixed effects interacted with Census division. Observation counts rounded for disclosure avoidance purposes.

3.2 Other Outcome Variables

Table 6 evaluates the effects of Great Recession shocks on other related outcome variables. Given the negative effects on marriage and cohabitation, one may expect that exposure to larger unemployment shocks depressed fertility as well. To study this, I look at the effects of the Recession on total number of children[22] and fertility (whether a female respondent had a child in the previous year). The second and third columns of Table 6 report estimates

[22]Obtaining number of children requires linking parents and children within households in the data. I do this with code that constructs these linkages from Census project 1284 generously shared by Martha Bailey and Bryan Stuart.

Table 6: Effects on Alternate Outcome Variables

VARIABLES	(1) Number of Children	(2) Fertility	(3) Live w/ Parents	(4) Never Married
Age 21-23	-0.001	0.045	0.460	-0.043
	(0.002)	(0.101)	(0.294)	(0.213)
Age 24-26	-0.003	-0.056	0.335	0.390
	(0.004)	(0.111)	(0.319)	(0.399)
Age 27-29	-0.004	-0.214	-0.140	0.445
	(0.006)	(0.120)	(0.224)	(0.362)
Age 30-32	-0.006	-0.261	-0.298	0.597
	(0.008)	(0.139)	(0.214)	(0.324)
Age 33-35	-0.012	-0.125	-0.220	0.811
	(0.008)	(0.247)	(0.262)	(0.256)
Observations	2252000	1121000	2252000	2252000
R^2	0.179	0.011	0.156	0.201

Notes: Table presents estimates of β_a from equation (2). Standard errors clustered at the year-2000 CZ level and are in parentheses. The sample includes individuals of the 1983-1987 birth cohorts observed in the 2005-2018 ACS and linked to themselves in the 2000 Decennial Census; see text for details. All specifications include demographic controls, year fixed effects, CZ fixed effects, and age fixed effects interacted with Census division. Observation counts rounded for disclosure avoidance purposes.

of β_a for these outcome variables. I detect negative effects on fertility that are similar in magnitude to other estimates in the literature[23] but are imprecisely estimated.

Next, I consider whether larger recession exposure is associated with a higher likelihood of cohabiting with one's parents, which I define as whether an individual is coded as being the child of the household head in the ACS. This potential response may be a mechanism for depressing marriage and has important implications for the impacts of the Great Recession on consumption. I find limited evidence that exposure to larger recession shocks increased

[23]The age 33-35 estimate for number of children suggests that a one percentage point increase in the unemployment faced by somebody in their early 20s results in -0.012 children, which translates to about 12 fewer children per 1,000 individuals. This is quite comparable to Currie and Schwandt (2014)'s estimate of 14.6 fewer conceptions using a similar specification with older birth cohorts.

parental cohabitation among individuals in their early-to-mid 20s, but the effects are noisily estimated and appear to fade over time fairly quickly[24].

Finally, the use of marriage as the outcome variable in the baseline specification means that I cannot distinguish between individuals who are divorced and those who are never married. To observe whether this distinction is meaningful, I use an indicator for never being married as an alternate outcome variable. The results using this variable are virtually identical to the baseline: a 1 pp larger shock increases the probability that one is never married by age 33-35 by 0.811 percentage points.

4 Implications for Consumption

The previous section demonstrated that the Great Recession had a statistically and substantively significant negative scarring effect on marriage among young adults. To what extent might this magnify the impact the Great Recession had on consumption for these individuals? Given that couples may pool resources and share consumption to a substantial extent, even a small reduction in partnering could have considerable implications for economic well-being. In this section, I conduct a simple exercise to estimate these potential implications under a range of scenarios[25].

Suppose that an agent consumes their wage earnings W hand-to-mouth. Denote the scarring effect of being exposed to a worse Recession shock on earnings as Δ_W. If we ignore any intra-household sharing and assume that agents may only use their personal earnings to finance consumption, then the scarring effect of the Great Recession on consumption Δ_C is simply equal to the scarring effect on individual earnings, or:

$$\Delta_C = W - (W - \Delta_W) = \Delta_W.$$

Now consider the possibility for married agents to share consumption. Suppose that agents are married with probability P and each derive consumption from household earnings

[24]Tables B.5 through B.8 present heterogeneity in effects for number of children and parental cohabitation by demographics and socioeconomic background. Fertility effects appear to be relatively concentrated on non-white women and individuals from poor to middle-class backgrounds. The effects on parental cohabitation are in general quite noisily estimated.

[25]Note that focusing on long-term scarring effects simplifies things by allowing me to ignore short-term concerns such as the cyclicality of rent or cohabitation with parents. In particular, evidence has shown that the latter response may be important in smoothing short-term consumption for young adults when exposed to labor market shocks (Kaplan, 2012). However, the results in Table 6 suggest that such responses fade over time.

W^H according to

$$\frac{W^H}{\phi},$$

where ϕ is the equivalence scale and the key parameter of this exercise. Intuitively, ϕ governs the extent to which individuals within a household share consumption: if $\phi = 2$, then the agent and their spouse do not share at all and each consume half of household income. If $\phi = 1$, then the two share perfectly. Given a value of P, in the baseline world the agent can expect to consume

$$C^B = (1 - P) \cdot W + P \cdot \left(\frac{W^H}{\phi} \right).$$

Now denote the scarring effect of the Great Recession on marriage probability as Δ_M. If both members of a married couple receive the same earnings shock from the Recession[26], expected consumption for an individual agent following a one-point larger Recession shock is given by

$$C^R = (1 - P + \Delta_M) \cdot (W - \Delta_W) + (P - \Delta_M) \cdot \left(\frac{W^H - 2\Delta_W}{\phi} \right),$$

and the Recession's impact on consumption is then

$$\Delta_C = C^B - C^R.$$

Intuitively, the impact of the recession on consumption now depends on its effects on both earnings and marriage probabilities. The base rate of marriage in conjunction with the household equivalence scale ϕ also influences the extent to which earnings losses translate to decreases in consumption[27].

Following the empirical results, I use $\Delta_W = \$700$ and $\Delta_M = 0.008$. From the 2018 American Community Survey (Ruggles et al., 2020), I set $P = 0.546$ and $W = \$37,471$. I also consider different potential consumption implications for men and women, which may be important given gender earnings differences. For men and women, I use $W = \$44,381$ and $W = \$30,459$ respectively.

[26] Table B.3 does not suggest meaningful heterogeneity over gender for Great Recession scarring effects on earnings; Rinz (2019) finds similar results for Millenials.

[27] A notable assumption in this is that the ϕ for marginal marriages affected by the recession is the same for non-marginal marriages. To the extent that this may not be true, larger values of ϕ may be more appropriate for this exercise. I also focus exclusively on consumption here and abstract away from utility or the decision to marry in the first place — indeed, if individuals only derive utility from consumption according to some concave function, they may be more willing to become married after a negative income shock.

Table 7: Consumption Impacts from Marriage Scarring Effects

Panel A: Equivalence Scales (values of ϕ)							
Family Size	OECD	NAS	HHS	DOC	LM	Nelson	**Mean**
1	1	1	1	1	1	1	**1**
2	1.7	1.62	1.34	1.28	1.06	1.06	**1.34**
3	2.20	2.00	1.68	1.57	1.28	1.17	**1.65**

Panel B: Consumption Scarring Effects (Individual Earnings Effect = \$700)							
Scale	OECD	NAS	HHS	DOC	LM	Nelson	**Mean**
Scenario 1: No Children							
Average	\$819	\$859	\$1,033	\$1,080	\$1,300	\$1,300	**\$1,065**
Men	\$764	\$803	\$977	\$1,025	\$1,244	\$1,244	**\$1,010**
Women	\$875	\$914	\$1,089	\$1,136	\$1,355	\$1,355	**\$1,121**
Scenario 2: Child if Married							
Average	\$639	\$700	\$829	\$885	\$1,142	\$1,180	**\$896**
Men	\$583	\$644	\$773	\$829	\$1,086	\$1,124	**\$840**
Women	\$694	\$756	\$884	\$941	\$1,197	\$1,235	**\$951**
Scenario 3: Child Regardless							
Average	\$629	\$691	\$823	\$880	\$1,140	\$1,178	**\$890**
Men	\$596	\$656	\$781	\$836	\$1,087	\$1,126	**\$847**
Women	\$661	\$725	\$864	\$923	\$1,193	\$1,231	**\$933**

Notes: All scarring effects in 2012 dollars. Panel A displays different estimates of household equivalence scales ϕ used in the economics literature. Panel B displays different values of Δ_C depending on equivalence scale used and scenario. Scenario 1: no child in household regardless of whether agent is married or unmarried. Scenario 2: one child in household if agent is married, otherwise no child. Scenario 3: one child in household regardless if agent is married or unmarried. Sources of equivalence scales as follows: OECD: Organization for Economic Cooperation and Development (1982); NAS: Citro and Michael (1995); HHS: Federal Register (1991); DOC: U.S. Department of Commerce (1991); LM: Lazear and Michael (1980); Nelson: Nelson (1993).

Table 7 presents the results of this exercise (that is, estimates of Δ_C) for a variety of combinations of scenarios and values of household equivalence scales ϕ. The range of values of ϕ reported in Panel A are the same used in Fernández-Villaverde and Krueger (2007) and come from a variety of expert evaluations or econometric estimates[28]. The first scenario considers the Recession's scarring effect on individual consumption if an agent has no children in the household regardless of whether they are married. For the average person, the consumption effect after taking into account reduced marriage rates and averaging over different equivalence scale estimates is $1,065, approximately 50% larger than the individual earnings scarring effect of $700. Gender earnings gaps result in decreased household formation reducing the consumption of women more than men.

The next two scenarios account for the possibility of children being present in the household — since spouses share consumption with their children as well as with each other, focusing on only spousal income sharing may overstate the consumption impact of marriage reductions following the Great Recession. In scenario 2, I consider the case of an agent who has a child if they are married and is childless if they are not. Since I do not find statistically significant effects of the Recession on fertility, I additionally consider a scenario where the agent has a child in the household regardless of marital status. In both these scenarios, the scarring effect of the Recession attenuates substantially but on average is still close to 30% larger than the individual earnings effect.

Of course, a household may have multiple children, and married agents may choose to opt out of the labor force when they would not have were they single. Considering all possible scenarios and trying to derive a single estimate of Δ_C would require a more complicated model of household formation and intra-household bargaining and is beyond the scope of this exercise. Rather, this exercise is intended to demonstrate that the scarring effects of the Great Recession on household formation are likely to have implications for consumption and economic well-being that individual earnings scarring effects cannot capture[29].

[28] Specifically, the estimates come from the OECD (Organization for Economic Cooperation and Development, 1982), the Panel on Poverty and Family Assistance of the National Academy of Sciences (Citro and Michael, 1995), the Department of Health and Human Services (Federal Register, 1991), the Department of Commerce (U.S. Department of Commerce, 1991), Lazear and Michael (1980), and Nelson (1993).

[29] Another potential factor is that individuals who were deterred from forming families due to the recession may have been less able to buy homes. While homeownership and house value are available in the ACS, disentangling the effect of reduced family formation on these variables from both personal income shocks induced by the recession and the fact that CZs that saw larger unemployment shocks also saw larger decreases in house values would be difficult. Fisher and Gervais (2011) estimate using the National Longitudinal Survey of Youth 1979 cohort that being married increases the likelihood of homeownership by 23 pp, which would suggest that the marriage scarring effect of 0.8 pp may have decreased homeownership by 0.184 pp.

5 Discussion and Conclusion

This paper attempts to make several contributions. First, this is one of the first papers to study the scarring effects of exposure to the most severe economic meltdown in close to a century on non-economic outcomes: in my empirical analysis, I document that a one pp larger CZ unemployment shock suffered by young adults in the Great Recession is associated with a 0.8 pp reduction in marriage rates in their early-to-mid 30s, with particularly strong effects for poor to middle-class whites. The estimated effects for fertility, while negative, are imprecise. The lack of effects for individuals from high-income families suggests that one impact of the Great Recession may have been to exacerbate socioeconomic inequalities in family structure, which itself may have important implications for economic mobility in the United States.

The empirical analysis attempts to make a modest methodological contribution by demonstrating the use of newly available data from U.S. Census composed of individuals in the American Community Surveys linked to themselves in the 2000 Decennial Census, which provides both enhanced geographic information on ACS respondents as well as socioeconomic background information for a subset of the sample. Many of the results that using my full sample are quite comparable to papers that have used the public-use version of the data to study earlier birth cohorts (Currie and Schwandt, 2014; Schwandt and VonWachter, 2020), suggesting that state-level variation may be adequate in evaluating the long-run effects of exposure to worse labor markets in early adulthood. However, the socioeconomic background information available in the data may make my sample a useful resource for future research on intergenerational issues.

Lastly, I demonstrate that the Recession's impact on these supposedly non-economic factors have salient economic implications. The fact that members within a household can share consumption to a considerable extent means that focusing on individual earnings may understate the impact of the Great Recession on economic well-being. In a back-of-the-envelope exercise, I demonstrate that the reductions in marriage rates I find may increase the impact of the Recession on consumption by an additional 30 to 50 percent depending on the assumptions used.

The empirical analysis is limited in that I cannot view respondents in my cohorts past age 35. Given the very long-lived effects of exposure to the 1980 recession found in Schwandt and VonWachter (2020), it will be interesting to see both whether and the extent to which my findings persist into the future. Additionally, while the exercise I conduct attempts to

be flexible in the estimates of household equivalence scales and scenarios considered, a richer model of household dynamics may be able to provide a more precise estimate of the economic importance of the effects on the Great Recession on marriage rates while also providing more insights into the mechanisms at play that drive the results I find. Developing such a model and using the estimates here to better understand how individuals weigh economic factors when choosing partners may be worthwhile.

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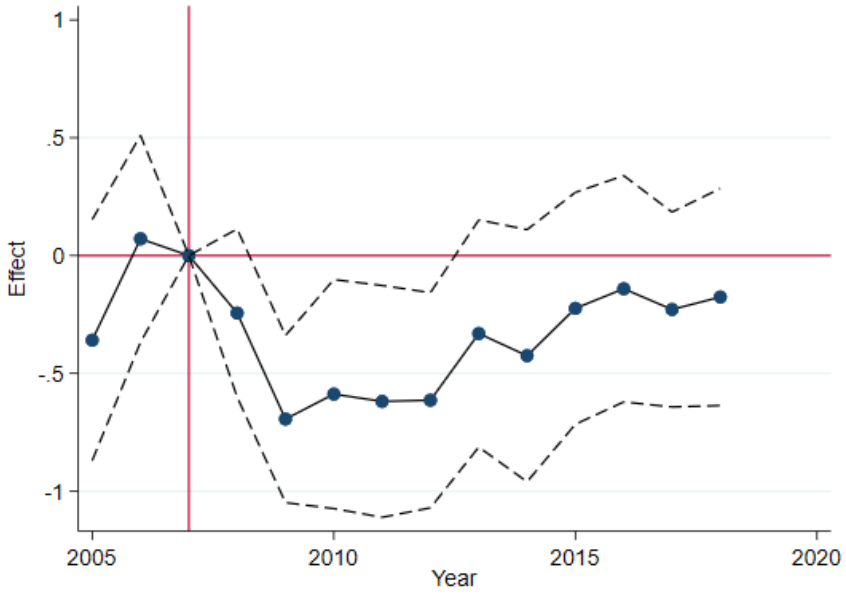
A Divisional Groupings of States

- **New England (NE):** Connecticut, Maine, Massachusetts, New Hampshire, Rhode Island, Vermont.
- **Mid-Atlantic (MA):** New Jersey, New York, Pennsylvania.
- **East North Central (ENC):** Illinois, Indiana, Michigan, Ohio, Wisconsin.
- **West North Central (WNC):** Iowa, Kansas, Minnesota, Missouri, Nebraska, North Dakota, South Dakota.
- **South Atlantic (SA):** Delaware, Florida, Georgia, Maryland, North Carolina, South Carolina, Virginia, District of Columbia, West Virginia.
- **East South Central (ESC):** Alabama, Kentucky, Mississippi, Tennessee.
- **West South Central (WSC):** Arkansas, Louisiana, Oklahoma, Texas.
- **Mountain (MO):** Arizona, Colorado, Idaho, Montana, Nevada, New Mexico, Utah, Wyoming.
- **Pacific (PA):** Alaska, California, Hawaii, Oregon, Washington.

B Supplementary Tables and Figures

Figure B.1: Effects of Great Recession Exposure on Employment



Notes: Figures present estimates of β_t from equation (1). Standard errors clustered at the year-2000 CZ level, and 95% confidence intervals shown. The sample includes individuals of the 1983-1987 birth cohorts observed in the 2005-2018 ACS and linked to themselves in the 2000 Decennial Census; see text for details. All specifications include demographic controls, CZ fixed effects, age fixed effects, and year fixed effects interacted with Census division.

Figure B.2: Effects of Great Recession Exposure on Earnings and Marriage by Year, Older Cohorts

(a) Wage Earnings

(b) Married

Notes: Figures present estimates of β_t from equation (1). Standard errors clustered at the year-2000 CZ level, and 95% confidence intervals shown. The sample includes individuals of the 1973-1977 birth cohorts observed in the 2005-2018 ACS and linked to themselves in the 2000 Decennial Census; see text for details. All specifications include demographic controls, CZ fixed effects, age fixed effects, and year fixed effects interacted with Census division.

Table B.1: Robustness of Marriage Results

	(1)	(2)	(3)	(4)
VARIABLES	Married	Married	Married	Married
Age 21-23	0.022	-0.406	-0.118	-0.042
	(0.153)	(0.145)	(0.180)	(0.168)
Age 24-26	-0.277	-1.024	-0.427	-0.366
	(0.277)	(0.216)	(0.320)	(0.307)
Age 27-29	-0.435	-1.089	-0.469	-0.485
	(0.225)	(0.243)	(0.303)	(0.295)
Age 30-32	-0.627	-1.009	-0.526	-0.619
	(0.184)	(0.219)	(0.254)	(0.254)
Age 33-35	-0.789	-0.979	-0.615	-0.741
	(0.189)	(0.232)	(0.234)	(0.232)
Interaction	Marriage	College	Employment	Poverty
Observations	2252000	2252000	2252000	2252000
R^2	0.161	0.161	0.161	0.161

Notes: Table presents estimates of β_a from equation (2) while additionally controlling for indicated pre-recession CZ characteristic interacted with age effects. Standard errors clustered at the year-2000 CZ level and are in parentheses. The sample includes individuals of the 1983-1987 birth cohorts observed in the 2005-2018 ACS and linked to themselves in the 2000 Decennial Census; see text for details. All specifications include demographic controls, CZ fixed effects, year fixed effects, and age fixed effects interacted with Census division. Observation counts rounded for disclosure avoidance purposes.

Table B.2: Additional Results

VARIABLES	(1) Married	(2) Married	(3) Married	(4) Wage Earnings	(5) Employed	(6) Married
Age 21-23	0.015	-0.011	-0.156	-85.18	-0.220	-0.107
	(0.187)	(0.179)	(0.237)	(43.45)	(0.190)	(0.165)
Age 24-26	-0.243	-0.350	-0.417	-415.1	-0.498	-0.680
	(0.357)	(0.334)	(0.371)	(99.88)	(0.272)	(0.267)
Age 27-29	-0.388	-0.510	-0.389	-603.5	-0.259	-0.866
	(0.321)	(0.300)	(0.372)	(206.5)	(0.356)	(0.257)
Age 30-32	-0.565	-0.704	-0.469	-763.7	-0.093	-1.120
	(0.260)	(0.241)	(0.338)	(269.2)	(0.321)	(0.232)
Age 33-35	-0.735	-0.853	-1.316	-787.0	-0.252	-1.155
	(0.231)	(0.222)	(0.345)	(345.9)	(0.292)	(0.209)
CZ	2000	2000	2000	2000	2000	Current
Additional Long-Form Controls	—	—	No	—	—	—
Education Dummies	All	None	College	College	College	College
Observations	2252000	2252000	537000	2252000	2252000	4832000
R^2	0.163	0.159	0.155	0.277	0.065	0.208

Notes: Table presents estimates of β_a from equation (2). Standard errors clustered at the year-2000 CZ level and are in parentheses. The sample includes individuals of the 1983-1987 birth cohorts observed in the 2005-2018 ACS and linked to themselves in the 2000 Decennial Census; see text for details. All specifications include demographic controls, CZ fixed effects, year fixed effects, and age fixed effects interacted with Census division. Observation counts rounded for disclosure avoidance purposes.

Table B.3: Demographic Heterogeneity in Effects on Wage Earnings

VARIABLES	(1) Earnings	(2) Earnings	(3) Earnings	(4) Earnings
Age 21-23	-213.5	-68.6	-84.5	144.2
	(71.7)	(48.3)	(118.8)	(80.0)
Age 24-26	-597.8	-264.7	-210.7	-285.9
	(110.4)	(110.9)	(152.9)	(180.2)
Age 27-29	-619.1	-302.8	-797.8	-381.3
	(223.4)	(179.4)	(317.2)	(316.8)
Age 30-32	-713.8	-530.7	-736.1	-531.3
	(330.3)	(246.0)	(366.4)	(380.7)
Age 33-35	-562.8	-662.0	-705.0	-687.7
	(367.9)	(279.4)	(616.2)	(561.1)
Sample	W Men	W Women	NW Men	NW Women
Observations	873000	863000	258000	258000
R^2	0.29	0.27	0.26	0.27

Notes: W: non-Hispanic white. NW: nonwhite or Hispanic. Table presents estimates of β_a from equation (2). Standard errors clustered at the year-2000 CZ level and are in parentheses. The sample includes individuals of the 1983-1987 birth cohorts observed in the 2005-2018 ACS and linked to themselves in the 2000 Decennial Census; see text for details. All specifications include demographic controls, CZ fixed effects, year fixed effects, and age fixed effects interacted with Census division. Observation counts rounded for disclosure avoidance purposes.

Table B.4: Socioeconomic Heterogeneity in Effects on Wage Earnings

VARIABLES	(1) Earnings	(2) Earnings	(3) Earnings	(4) Earnings
Age 21-23	-98.6	-135.5	-260.4	-75.0
	(143.5)	(148.9)	(155.7)	(156.0)
Age 24-26	-569.3	-501.5	-579.0	-277.5
	(171.6)	(197.7)	(178.3)	(168.5)
Age 27-29	-43.7	-434.4	-765.4	-536.3
	(183.9)	(194.5)	(192.6)	(280.7)
Age 30-32	-485.2	-599.8	-1,046	-1,215
	(197.7)	(274.9)	(224.8)	(315.9)
Age 33-35	-1,260	-1,033	-298.2	-199.9
	(433.0)	(423.0)	(403.3)	(539.9)
Parent Income Quartile	1	2	3	4
Observations	110000	142000	152000	132000
R^2	0.21	0.23	0.26	0.31

Notes: Table presents estimates of β_a from equation (2). Standard errors clustered at the year-2000 CZ level and are in parentheses. The sample includes individuals of the 1983-1987 birth cohorts observed in the 2005-2018 ACS and linked to themselves in the 2000 Long-Form Decennial Census; see text for details. All specifications include demographic controls, year fixed effects, CZ fixed effects, and age fixed effects interacted with Census division. Observation counts rounded for disclosure avoidance purposes.

Table B.5: Demographic Heterogeneity in Effects on Fertility

VARIABLES	(1) # Children	(2) # Children	(3) # Children	(4) # Children
Age 21-23	0.001	-0.001	-0.002	- 0.009
	(0.002)	(0.002)	(0.002)	(0.006)
Age 24-26	-0.002	-0.006	0.000	-0.009
	(0.004)	(0.005)	(0.005)	(0.008)
Age 27-29	-0.001	-0.004	-0.002	-0.008
	(0.006)	(0.007)	(0.008)	(0.010)
Age 30-32	-0.006	-0.005	0.007	-0.011
	(0.008)	(0.008)	(0.008)	(0.012)
Age 33-35	-0.010	-0.006	0.010	-0.037
	(0.009)	(0.009)	(0.015)	(0.014)
Sample	W Men	W Women	NW Men	NW Women
Observations	873000	863000	258000	258000
R^2	0.152	0.215	0.108	0.186

Notes: W: non-Hispanic white. NW: nonwhite or Hispanic. Table presents estimates of β_a from equation (2). Standard errors clustered at the year-2000 CZ level and are in parentheses. The sample includes individuals of the 1983-1987 birth cohorts observed in the 2005-2018 ACS and linked to themselves in the 2000 Decennial Census; see text for details. All specifications include demographic controls, CZ fixed effects, year fixed effects, and age fixed effects interacted with Census division. Observation counts rounded for disclosure avoidance purposes.

Table B.6: Demographic Heterogeneity in Effects on Parental Cohabitation

VARIABLES	(1) LWP	(2) LWP	(3) LWP	(4) LWP
Age 21-23	0.700	0.232	0.321	-0.155
	(0.342)	(0.320)	(0.419)	(0.480)
Age 24-26	0.558	-0.123	0.015	0.009
	(0.304)	(0.264)	(0.525)	(0.411)
Age 27-29	-0.073	-0.621	0.012	-0.612
	(0.232)	(0.246)	(0.447)	(0.394)
Age 30-32	-0.140	-0.687	-0.102	-0.708
	(0.233)	(0.280)	(0.471)	(0.408)
Age 33-35	-0.134	-0.680	-0.561	0.204
	(0.291)	(0.309)	(0.521)	(0.612)
Sample	W Men	W Women	NW Men	NW Women
Observations	873000	863000	258000	258000
R^2	0.174	0.162	0.117	0.122

Notes: LWP: Live with parents. W: non-Hispanic white. NW: nonwhite or Hispanic. Table presents estimates of β_a from equation (2). Standard errors clustered at the year-2000 CZ level and are in parentheses. The sample includes individuals of the 1983-1987 birth cohorts observed in the 2005-2018 ACS and linked to themselves in the 2000 Decennial Census; see text for details. All specifications include demographic controls, CZ fixed effects, year fixed effects, and age fixed effects interacted with Census division. Observation counts rounded for disclosure avoidance purposes.

Table B.7: Socioeconomic Heterogeneity in Effects on Fertility

VARIABLES	(1) # Children	(2) # Children	(3) # Children	(4) # Children
Age 21-23	-0.015	-0.008	-0.001	0.004
	(0.008)	(0.006)	(0.004)	(0.003)
Age 24-26	-0.002	-0.016	0.001	-0.001
	(0.009)	(0.008)	(0.006)	(0.005)
Age 27-29	-0.006	-0.023	0.002	-0.002
	(0.012)	(0.010)	(0.009)	(0.009)
Age 30-32	0.003	-0.022	-0.010	-0.009
	(0.013)	(0.011)	(0.011)	(0.012)
Age 33-35	-0.027	-0.037	-0.034	0.004
	(0.020)	(0.020)	(0.021)	(0.018)
Parent Income Quartile	1	2	3	4
Observations	110000	142000	152000	132000
R^2	0.179	0.181	0.193	0.195

Notes: Table presents estimates of β_a from equation (2). Standard errors clustered at the year-2000 CZ level and are in parentheses. The sample includes individuals of the 1983-1987 birth cohorts observed in the 2005-2018 ACS and linked to themselves in the 2000 Long-Form Decennial Census; see text for details. All specifications include demographic controls, year fixed effects, CZ fixed effects, and age fixed effects interacted with Census division. Observation counts rounded for disclosure avoidance purposes.

Table B.8: Socioeconomic Heterogeneity in Effects on Parental Cohabitation

VARIABLES	(1) LWP	(2) LWP	(3) LWP	(4) LWP
Age 21-23	1.022	0.839	1.341	0.548
	(0.593)	(0.534)	(0.529)	(0.661)
Age 24-26	1.287	-0.075	0.381	-0.032
	(0.609)	(0.614)	(0.633)	(0.680)
Age 27-29	-0.061	0.581	-0.089	0.215
	(0.624)	(0.537)	(0.580)	(0.498)
Age 30-32	0.283	0.520	0.399	-0.399
	(0.597)	(0.470)	(0.528)	(0.572)
Age 33-35	0.476	0.876	0.329	-0.613
	(0.822)	(0.709)	(0.699)	(0.687)
Parent Income Quartile	1	2	3	4
Observations	110000	142000	152000	132000
R^2	0.122	0.152	0.177	0.181

Notes: LWP: Live with parents. Table presents estimates of β_a from equation (2). Standard errors clustered at the year-2000 CZ level and are in parentheses. The sample includes individuals of the 1983-1987 birth cohorts observed in the 2005-2018 ACS and linked to themselves in the 2000 Long-Form Decennial Census; see text for details. All specifications include demographic controls, year fixed effects, CZ fixed effects, and age fixed effects interacted with Census division. Observation counts rounded for disclosure avoidance purposes.